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Author(s): John Levi Martin

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What Is Field Theory?¹

John Levi Martin
University of Wisconsin, Madison

Field theory is a more or less coherent approach in the social sciences whose essence is the explanation of regularities in individual action by recourse to position vis-à-vis others. Position in the field indicates the potential for a force exerted on the person, but a force that impinges “from the inside” as opposed to external compulsion. Motivation is accordingly considered to be the paramount example of social structure in action, as opposed to a residue of chance or freedom. While field theory is often castigated for its necessarily tautological definition, this may be far more of an advantage than a defect. Field theory offers social scientists a combination of analytical insight and attention to the concrete; further, the implicit definition of “explanation” that it brings is one that, unlike conventional sociological definitions, is internally consistent and in accord with everyday usage.

THE PASSING CRISIS IN WESTERN SOCIOLOGY

Surveying the state of Western sociology at the dawn of the new millennium, what is most striking and perhaps troubling is the absence of theoretical crisis: even the most sour doomsayer cannot in good conscience point to any signs that there is a deep theoretical rupture or confusion in academic sociology as it currently stands, nor is there reason to suspect crisis looming in the near future. What has happened to the “perpetual youth” supposedly granted the social sciences (Weber [1904] 1949, p. 104)

¹ I have profited from the rancorous discussions of the Highland Park Colloquium on Theory, Methods, and Beer. I would also like to thank Neil Fligstein, Matt George, Ann Mische, and the reviewers for their probing criticisms that greatly increased the coherence of the argument, though all called for a more complete theoretical specification than I was able to provide. Finally, one can only acknowledge the loss of Pierre Bourdieu—it seems impossible to adequately describe how great a loss this is for the social sciences. Direct correspondence to John Levi Martin, Department of Sociology, University of Wisconsin, 1180 Observatory Drive, Madison, Wisconsin 53706. JLMartin@ssc.wisc.edu

that would forbid them from settling down into a consensual holding pattern?

It is not that the fundamental problems in social analysis have been resolved, namely (1) the absence of a clear criterion as to what constitutes good theory (some frequently heard, but frequently incompatible, standards are prediction of future states, parsimony, explanation of variance, reproducible intervention, intuitive accessibility, and the ability to sponsor generative research); (2) uncertainty as to the ontological status of key theoretical elements, not the least of which is society; and (3) frequent translation of social and political disagreements into seemingly scientific disputes regarding matters of fact. And yet all is quiet on the theoretical front.

I argue that this quiet has resulted from two seemingly welcome, but deeply pernicious, trends: (1) widespread agreement to compromise on both false and true dualisms alike and (2) theoretical inflation. Regarding the first, it has been common for recent discussions of practically any conventional opposition (the list includes but is not limited to macro/micro, social/individual, nature/nurture, static/dynamic, structure/agency, quantitative/qualitative) to conclude with a resounding verdict of “both.” *Both* the individual *and* the social are important determinants of X, Y, and Z. Without belittling the wisdom of such statements, such facile solutions (which Goldstone [1991, p. 49] terms “wishy-washy”) seem to allow the instantaneous dissolving of what for centuries have been understood as profound antinomies; perhaps more than the words “both” and “and” are required before we break off into small groups and celebrate, akin to the “mutual reconciliation societies” that Marx ([1843] 1977, p. 88) saw in Hegel’s understanding of “contradictions.”

Some, indeed most, of these dualisms may have been meaningless from the start; if the claims below are accepted, “structure” and “agency” would be one. In that case, a “both/and” approach, no matter how theoretically elegant (e.g., Sewell 1992) only furthers the hypostatization of meaningless terminology. Others may be theoretically unproductive or ambiguous (e.g., social/individual), yet tap fundamental questions that inspired the development of sociological thought in the first place. The tension between individual- and social-level understandings—the former intuitively accessible to our “first person” understandings in terms of motivations and constraints, the latter satisfying the scientific yearning for a wholly “third person” explanation—has been one of the most productive tensions in sociological theory, even if it is in principle irresolvable. A premature defusing of this tension robs us of an important incentive to theorize, bringing us to the second point.

This widespread détente has led to the promotion of what would otherwise be seen as important empirical generalizations, or statements re-

garding the scopes and conditions of such generalizations, to the status not only of “theories,” but even theories worthy of names written in title case (the ideational equivalent of a named chair). Since *both* structure *and* agency are present in, say, social movements, there is little need to consider whether a theory as grand and as deliberately indifferent to intentionality as that of Marx and Engels is correct; the word “theory” then becomes available for more modest—and more promising—investigations. But if every case of successful data analysis becomes a theory, and only successful analyses see the light of day, we are destined to a seemingly endless proliferation of theories. Indeed, it might be better to replace the word “theory” in such cases with “work group” or “cluster” (in Terry Clark’s [1973] sense), or even “my career.”

Unfortunately, Pierre Bourdieu’s seminal work—or at least, the introductions to such work—may have led field theory to become associated with the resolution of such dualisms and to be lumped in with what I am arguing are either premature defusings of important tensions or new-found alliances between imaginary opponents. I make the case that field theory is something quite different that has the potential to yield general but nontrivial insights into questions rightly deemed theoretical and to organize research in a productive fashion. Finally, field theory allows for the rigorous reflexivity that is necessary in all cases in which sociology attempts large-scale political and institutional analyses.

I begin by sketching the essences of field theory most generally, acknowledging its weaknesses and indeterminacies, and critically analyzing the degree to which field theory is applicable to the social world. I briefly discuss the major variants of field theory in the social sciences and highlight their common elements. I then argue that each of these has an important contribution for the social sciences. I conclude by suggesting that recent advances in one branch of field theory may be generalizable and that this gives us the possibility of joining the analytic insights given by field theory to a more grounded line of research.

ESSENCES OF FIELD THEORY

Some Characteristics of Field Theory

I will argue that there is a sufficiently distinct core to field theory, both in the social sciences and in other sciences, to warrant its being considered an approach or a family of approaches (also see Mey 1972). Field theory stems from the physical sciences; while there are a number of different fields, and theories of each have varied over the course of their development, the best model of intellectually rigorous field theory would be classical (nonrelativistic) electromagnetism, though the important features

here are found in similar systems (Newtonian gravitation has much in common with field theory, but only Einstein's theory of general relativity actually technically gave it a field theoretic form [Hesse 1970, p. 226]).

Field theories really took the basic form of the fluid mechanics developed in the 18th century, in which equations linked a "flow"—or potential for transmitted force—to spatial coordinates, but applied this form to situations where no fluid could be found; examples are motion induced by gravity, electricity, or magnetism (Hesse 1970, p. 181; Rummel 1975, p. 26; also cf. Köhler 1947, p. 127). I will follow general use and employ the term "field theory" to denote only those theories that do not involve a clearly existent substantial medium. Our discussion should begin with a careful examination of the characteristics of such field theories not because the physical sciences are in general a good model for the social sciences, but because if field theory has distinctive characteristics, they may have been most apparent in this realm. An examination of classical electromagnetism suggests that field theory may be said to have the following characteristics:

1. It purports to explain changes in the states of some elements (e.g., a static field induces motion in a charged particle) but need not appeal to changes in states of other elements (i.e., "causes").
2. These changes in state involve an interaction between the field and the existing states of the elements (e.g., a particle of positive charge moves one way and one of negative charge another; see Maxwell [1891] 1954, p. 68; Koffka 1935, p. 42; Köhler 1947, p. 300).
3. The elements have particular attributes that make them susceptible to the field effect (particles differ in the degree and direction of charge).
4. The field without the elements is only a potential for the creation of force, without any existent force (Hesse 1970, p. 196).
5. The field itself is organized and differential (Koffka 1935, p. 117). In other words, at any position the field is a vector of potential force and these vectors are neither identical nor randomly distributed.

It is worth pointing out how utterly at odds such a conception is with the conventional understanding of causality in the social sciences. According to this conception, elements have attributes, mutually exclusive attributes often being considered instances of a "variable." Relations between elements are interpreted as by-products of relations between variables, and causality is said to exist when a change in state in one variable produced by external manipulation would *impel* a change in state in another variable. Causality follows a mental image of external impulsion

taken from classical mechanics (basically the conception of Hobbes), but recasts this in terms of variables, as opposed to substances (see Abbott 1988*b*).

Our current social science methods are almost uniformly based on such epistemological assumptions and consequently form an inventory of ways of linking variation in one attribute to variation in another (where the attributes belong to the same units). Since sociologists tend to be suspicious of things that do not vary—after all, sociology’s claim to a domain distinct from those of biology and psychology largely rested on the irreducibility of variation—this methodological imperative has generally been a congenial one. But it is folly to go on to declare that the essence of explanation *is* explaining variation and that other approaches are nonscientific.

Lieberson (1985) gives the hypothetical example of a sociologist attempting to understand why things fall. Methodologically acute, our sociologist assembles a set of different objects: a cannonball, a feather, a potato, and so on, and begins to drop them, measuring their acceleration downward. Linking this acceleration (the “dependent variable”) to various attributes of the plummeting objects, such as volume, weight, composition, density (the “independent variables”) our researcher may (if lucky) come up with a rather large R^2 and conclude that he understands why things fall.

“What is going on here? Something must be wrong if social researchers think that they have a full grasp of falling objects without ever invoking gravity.” Lieberson (1985, p. 103) argues that this researcher has confused variation in the acceleration or accumulated velocity with the fact of acceleration itself—a constant and hence invisible to us. “What we get at is variation in the impact of the force. But we do not get at what the force is.”

Here Lieberson leaves matters, happy to have used this example to make an important point. But our researcher may, thus enlightened, now diligently go back to try to “get at” what this force is. Our researcher will not get very far. Few of us are Newtons, and even Sir Isaac did not feel that he had “gotten at” this force (Jammer 1957, p. 137). Furthermore, it is safe to say that no living human being has ever really “gotten at” this force. But are then further efforts necessarily in vain? In between the bumbling foolishness of our researcher’s first attempt, and the most sophisticated science in human history, lies field theory.

It was a protofield theory—including the postulation of an invisible “occult force”—that was able to explain regularities in acceleration due to gravity, both on earth and in the heavens. Field theory posits an enveloping gravitational field that we can neither see nor measure except via its effects, and instead of trying to maximize explained variance, proceeds by assuming in principle a perfectly simple determination. As

Ernst Cassirer said, “Galileo did not discover the law of falling bodies by collecting arbitrary observations of sensuously real bodies, but by defining hypothetically the concept of uniform acceleration” (Cassirer [1923] 1953, p. 354). Interestingly, this obsession with the distinctiveness of Galileo’s method²—and even more with Cassirer’s treatment of it—lies at the heart of field theory in the social sciences. (Cassirer influenced both Bourdieu and Lewin, and Lewin was in turn the influence for most other field theorists. Lewin cites the above passage in his tribute to Cassirer, from whom he took a course in philosophy [Lewin (1949) 1999*b*, p. 32; see also Marrow 1969, p. 9]).

Of course, one may be inspired by the approach of a scientist in another discipline without claiming that it is profitable to adopt as a guiding vision portions of another science. Yet field theorists have evidently believed that the above five distinctive points, transferred to the realm of social theory, are not only meaningful but helpful. I discuss each and its application to the social world in turn.

Explication of the Points with Reference to Social Phenomena

The first point was that field theory purports to explain changes in the states of some elements but involves no appeal to changes in states of other elements (“causes”; see Mey 1972, p. 7). Instead, one makes reference to a characteristic of the field in the position occupied by some element. This characteristic of the space is usually seen as a vector (Hesse 1970, p. 192), whatever it is called (“valence” in Lewin’s [1999*b*] terminology; “slope” or “gradient” in Spiegel [1961]). This type of explanation is clearly foreign to sociology—it is difficult to persuade others that one is able to explain, say, occupational mobility by making recourse to the fact that nothing else is changing. Yet we very well know that there are certain forms of upward mobility that are built into certain careers and in fact we expect that such mobility will tend to take place for persons in a certain position so long as “nothing happens.”

The second point was that these changes in state involve an interaction

² It is quite significant that this example of the inability for generalizing techniques to understand gravity was first used by Lewin ([1931] 1999*a*); it was also used by Brandt (1952, p. 47) in the first major introduction of field theory to the social sciences. For other field theoretic discussions of the importance of this example, see Mey (1972, pp. 92, 239). Interestingly, Galileo’s own impatience with field theories and their “occult properties” led him to castigate Kepler for believing the old “puerilities” of Ptolemy and Aquinas and linking the tides to the—obviously impossible—influence of the moon (Hesse 1970, pp. 126–27). But by conceiving of gravity as akin to magnetic lines of force, Kepler used this phenomenon of the tides to conceive of celestial gravity as a general attractive force (Jammer 1957, pp. 83, 89).

between the field and the existing states of the elements (Verschuur 1993, p. 101). The closely related third point was that the elements have particular attributes that make them susceptible to the field effect; the “force” that impinges upon some object in a field is a function both of the field effect, and of some characteristic of the object itself. Thus massless bodies remain unaffected by a gravitational field. There is no field known to physics that affects all particles; similarly, the mere existence of some class of persons who are not susceptible to a social field effect does not disprove the claims regarding the existence of the field. However, it must be possible to specify a priori which types of persons will be susceptible, just as we can say in advance that some substances will and others will not be affected by a magnetic field.

The fourth point was that the field without the elements is only a potential for the creation of force, without any existent force (Brandt 1952, p. 180). Thus the field explains the otherwise inexplicable transfer of energy to an element that is not necessarily in contact with any other element. Consequently, field theory is generally applicable for cases in which the alternative form of explanation involves action at a distance, a form of explanation that has generally been treated with suspicious dislike by Western (in contrast to Eastern, especially Chinese) science (Needham 1981, p. 14; though see Hesse 1970, p. 187). While the distinction may seem like hair splitting, a field replaces the idea of action at a distance, in which X somehow *directly* affects some Y that it does not touch, with a purely *local* explanation (see Maxwell 1954, p. 70; see also the discussion of Koffka 1935, p. 41). The field directly induces a potential energy in Y; the presence of a continuous medium like a fluid is sufficient but not necessary for such local action (here I rely on Schwinger et al. [1998, pp. 2–5]; Hesse [1970, pp. 195, 201]; or cf. Maxwell [1954, pp. ix, 67], Mey [1972, p. 8]). Although X may somehow “cause” or anchor the field, we do not say that X itself affects Y. The potential for force is in the field, not in the magnet (Verschuur 1993, p. 98; cf. Marrow 1969, p. 31).

As a consequence, the field itself is not directly measurable; its existence can only be proved by its effects (Rummel 1975, p. 27). Because of this, and the more general Western discomfort with any explanation not ultimately reducible to hard particles whamming into one another, analysts generally only propose field theories when they have run out of other options. (Newton is the classic example in physics, but it was a similar need that led to *Gestalt* theory, as we shall see below.)

The last point was that the field itself is organized and differential (Brandt 1952, p. 183). The field may frequently be seen in topological terms of some sort, since its variations may be understood as variations in the strength and direction of motion induced in a particle. Thus at any

point, then, the field consists of a slope (a gradient) down which an object will “roll” (cf. Gibson 1986, pp. 151–52). In the social sciences, the field serves as some sort of representation for those overarching social regularities that may also be visualized (by competing theoretical orientations) as quasi-organisms, systems, or structures.

Field theory, then, has several generic characteristics no matter what the domain of application. Consequently, it seems reasonable to evaluate the general strengths and weaknesses of field theory as an explanatory approach before proceeding to propose its use for the social sciences. Since the limitations and weaknesses of field theory have been pointed out before, I review them briefly, acknowledging their import but arguing that they are not sufficient to dissuade us from the project.

LIMITATIONS OF FIELD THEORY

Field Theory, Tautology, and Occultism

Perhaps the biggest danger of field theory is a tendency toward tautology:—since fields are only known by their effects (see Hesse [1970], pp. 135, 141 for a more subtle exposition), it is tempting to proliferate invisible fields that “explain” whatever it is that we otherwise cannot explain. For example, Faraday developed his idea of “lines of force” on the basis of experimental effects, and he developed the idea of a field on the basis of the patterns made by iron filings on paper under which magnets were placed (Verschuur 1993, pp. 82–83, 99). In this case, the parsimony and (after Maxwell) theoretical consistency of the posited field was sufficient to justify its theoretical use (Hesse 1970, p. 202). In other cases, however, we may judge the field theory proposed neither simpler than the data it is supposed to explain, nor to have sufficient intuitive accessibility.³ But even when field theories have incontestable explanatory power, they have frequently been opposed because they violate the assumptions of the mechanistic materialism that was the largely dominant metaphysics in the early modern scientific West (see Burt 1927).

The most important of these assumptions is that all creation or transmission of force must be explicable in terms of contact—in Leibniz’s

³ In *Two Great Systems* Galileo’s alter-ego asks his interlocutor (Simplicitus) about why things fall, and Simplicitus replies that everyone knows that, the answer is gravity. The former replies, “You should say that everyone knows that it is *called* gravity; but I do not question you about the name, but about the essence of the thing” (cited in Burt 1927, p. 100). As suggested by this interchange, a field theory that only names the field cannot be considered a theory, but as Hesse (1970, p. 253, cf. p. 197) says, “The charge of untestability is not always a capital one, particularly not in the case of new theories which are establishing a new fundamental model.”

words, that “a body is never moved naturally, except by another body which touches it and pushes it” (Hesse 1970, pp. 106, 157–59).⁴ Because field theories dispense with such mechanical contact, they are generally received with discomfort and with attempts to introduce substantial ethers that can more “scientifically” explain the observed effect. (Newton himself, unable to dismiss mechanistic criticisms of his conception of gravity, later added an “explanation” in terms of an ether composed of mutually repelling particles [Westfall 1977, p. 157].⁵) An ether differs from the fluids of fluid dynamics in being a medium that responds as if it were a fluid, but apparently has the ability to penetrate any other object (e.g., Huygens’s proposed gravitational ether [Jammer 1957, pp. 114–15, cf. pp. 139, 141]). Such ethers are clearly just as nebulous as the field and, unless one dogmatically holds that all that exists must be treated as substance, have no scientific virtues. They needlessly complicate without adding to the explanation. Indeed, sociology has had its share of such unproductive ethers, from Parsons’s (1951) various media of exchange to the ubiquitous “power” of Foucault (1979).⁶

Even theorists applying field terminology to the social sciences (e.g., Brandt 1952, pp. 178, 180) admit that there is a potential problem in our inability to say exactly how some “force” is being transmitted. The fact

⁴ This idea goes back to Aristotle’s definition of local action (see Jammer 1957, pp. 36, 40, 60–62). While the West has generally been suspicious of all nonlocal effects, there have been periods in which action at a distance was considered theoretically acceptable (Hesse 1970, p. 187). Gilbert’s pioneering work on magnetism in 1600 led to the first such acceptance (Verschuur 1993, p. 38); Newton’s work led to a further acceptance of the “occult” phenomenon of gravity, though he himself was troubled by the lack of mechanism. Newton in his *Opticks*, qu. 31, noting that material bodies seem to have “certain Powers, Virtues, or Forces by which they act at a distance,” says, “These principles I consider not as Occult Qualities, supposed to result from the Specific Forms of things, but as General Laws of Nature, by which the things themselves are formed” (Newton [1730] 1952, pp. 401, 388; cf. Westfall 1977, p. 141). Note that “occult quality” was a technical term of the Aristotelians to denote qualities that were hidden in bodies and were responsible for manifest effects, an explanatory practice Newton abhorred. In contrast, he meant that we know the qualities of the objects (e.g., mass) but not why they lead to falling; “For these are manifest qualities, and their causes only are occult.” He reasonably points out that those who attempt to explain such forces (e.g., atomic attraction) with convenient mechanical claims such as “hooked atoms” are also inventing occult qualities.

⁵ It has been suggested by those attentive to Newton’s appreciation of alchemical reasoning that this protest may have been more strategic than genuine, and that indeed, it was the magical notions associated with the Hermetic tradition that allowed Newton to go beyond the mechanical philosophy (for a discussion, see Cohen 1994, p. 175).

⁶ While Foucault’s detailing of the many precise mechanisms that fall under the umbrella of “power” might suggest a fluid mechanics, the ability of this power to penetrate all barriers suggests that it is more akin to an ether than to a mechanically explicable fluid.

that Newton also could not say exactly what kind of force produces acceleration is of little comfort.⁷ Of course, it may be tempting to stress the heuristic nature of all theoretical concepts; such a recourse is considered epistemologically orthodox in the American sciences in general and sociology in particular due to the influence of the “operationalism” of Percy Bridgman (see Cartwright 1987).

But such a defense might be counterproductive and indeed undermine some of the coherence of field theory, which was, in the social sciences (as we shall see) largely derived from a general scientific trend in early 20th-century Germany that insisted that scientific theory had to “get at” the real world, not simply rearrange observations. This “getting at” the real world implied that the terms of the theory had to be intuitively accessible (*anschaulich*) as referring to a world we could understand and inhabit. (For the importance of *Anschaulichkeit* in the case of physics, see the classic work by Forman [1984]). The fact that field theory was rooted in this style of thought, which Harwood (1993) has called “comprehensive,” means that it is less than coherent to defend field theory by an appeal to heuristic (Ushenko 1958, p. 89).⁸

We are left with an apparent problem: field theory relies on something of uncertain ontological status, at least in the Western tradition where things that are real have to possess the properties of extension and mass. Such objects allow for explanation to proceed by a series of local collisions, the equivalent of which in the social sciences is explanation by recourse to “mechanisms.”

Between Mechanism and Function

Field theory, in contrast, emphatically does not attempt to give an explanatory account in terms of mechanisms (see Köhler 1947, p. 348). While the same might be said of most other sociological theories, such theories are not intrinsically at odds with mechanistic explanation, as is field theory.

⁷ Interestingly, with Einstein’s general relativity, such “forces” disappeared and instead were interpreted as simply the imposition of the wrong (i.e. Euclidian) coordinate system on a “warped” Riemann-like space (see Jammer 1957, p. 259f). The analogous re-interpretation for sociology would be (to anticipate) that there are no “social forces” external to people at all, simply “free agents” in social space warped by powerful institutions.

⁸ Interestingly, Gilbert, who pioneered the study of magnetism, also emphasized that while his conception was untestable, it offered intuitive accessibility, and Euler’s conception of pressure that led to a flexible fluid dynamics compatible with field theory succeeded precisely because, in contrast to Bernoulli’s, it was impressionistic and non-operational. Similarly, Faraday and Maxwell unashamedly relied on analogies to well understood processes (such as elasticity or fluid dynamics) that they did not mean to defend literally (see Hesse 1970, pp. 100, 191, 208–9).

By “mechanism,” sociologists generally mean to refer to some readily understandable causal sequence that explains some theoretically accounted-for pattern (Lundberg 1939, p. 375). While it has never been demonstrated that such mechanisms *must* be at a lower level of analysis than the theoretical units in question, this seems to be the case in practice.⁹

It is important not to confuse mechanisms with the theoretical claims themselves. Mechanisms are usually what is invoked when someone accepts a theoretical claim, but insists on asking “how” it comes to be the case.¹⁰ (In classic Lazarsfeldian survey analysis, a search for mechanism implied the use of intervening variables, though now such a search is more likely to lead to an appeal to a simplified accessory model.) While providing mechanisms is not necessary for a theory to be useful or correct, such provision often increases its plausibility. It is symmetrical to arguments that appeal to function, though in that case we generally attempt to explain “why” the accepted finding has to be the case.

As an example, consider the theory of evolution—that is, the claim that species change over time and that a range of species that originally included only very simple organisms developed into a range that went from the very simple to the fabulously complex. “Natural selection” is a mechanism that was offered by Charles Darwin to explain *how* evolution might actually occur. As this example makes clear, a successful mechanism need not be the empirical focus of work guided by a theory: evidence for or against evolution coming from the fossil record or overlaps in DNA rarely bears upon natural selection. Further, there is almost no empirical “evidence” for natural selection as “explaining” evolution. Finally, the introduction of this extremely reasonable mechanism has not led to any appreciable predictive power (though this is of course not always the case). Mechanisms, in sum, turn on making an accepted relation or set of relations plausible. In the case at hand, the intuitive accessibility of the mechanism of natural selection was considered by many sufficiently great to allow them to jettison theological or functionalist explanations of evolution.

Such theological or functionalist explanations, in stark contrast to mechanistic explanations, appeal to a *higher*—as opposed to a lower—level of analysis to explain a theoretical claim. Here we are totally uninterested in explaining “how” something comes to be: our claim is that it *must* be and that the mechanics are theoretically trivial. Mechanisms, in other

⁹ Definitions of mechanisms are generally poor; usage confirms my claims here (see, for example, Hedström and Swedberg 1998; Stinchcombe 1991; the classic use of such mechanisms is Schelling 1978).

¹⁰ There is a new interest in assembling explanations out of mechanisms without having an overarching theory; for examples, see Tilly (2000) and Mische (2003).

words, are a specific type of plausibility argument associated with reduction to potentially visible and understandable events—even if these events are never actually observed.

Field theories are peculiar in that they are incompatible with the specification of both functions and mechanisms. Somewhat formulaically, we may say that field theories, like mechanistic theories (and unlike functionalist theories), reach toward the concrete and propose only local action, but like functionalist theories (and unlike mechanistic theories), they insist that any case must be understood in terms of the global pattern. There are further relevant differences between field theory and both functional and mechanistic explanations. Regarding the first of these, the fact that the field at some place and time can be determined to be of a certain nature in no way implies that it *must* be this way—indeed, field theory, by never making explanation reach *outside* the field, must forswear any legitimating arguments that there is a reason why the field must be as it is. For this reason, field analysis is quite different from systems analysis, which, though it may stress the self-organization of the system, requires that the system be understood in contradistinction to an environment. But in field theory, explanation stops at the constitution of the field.¹¹

Regarding the latter, the incompatibility of field theory and mechanism does not arise because the sorts of phenomena treated by field theories cannot, in principle, be mechanically explicable—quite the contrary, it is this very possibility that repeatedly encourages speculative constructions of an ether or of a mechanistic plenum (space being completely filled with particles) to “explain” the field effect. But field theories are proposed, whether reluctantly or not, when no such mechanistic explanations currently offer promise: if there were a mechanism, there would be no need for a field theory. Accordingly, field theories may be seen as provisional theories that we are happy to replace when adequate knowledge of mechanisms is gained, should this be the case. Thus the field theory of electromagnetism was replaced with quantum electrodynamics, which was able to respecify the relations previously described in field terms as more “mechanistic” interactions between particles. Yet the very indeterminacy of the field theory was its strength: it became possible to explain “magnetism” wholly by recourse to the properties of the field, which could be treated as a thing in itself, as opposed to considering the properties of magnets (Verschuur 1993, pp. 99, 111, 121). The field displayed regularity and mathematically explicable properties; the lodestone remained confusing.

¹¹ For related reasons, field theory in the restricted sense used here does not generally examine the historical process whereby the field arose, although this may be a crucial question for the sociology of fields more generally (e.g., Abbott 1988a; Fligstein 1990).

Thus while the formal similarity of field theories to fluid dynamics encourages the search for an overlooked fluid that would allow the reduction of field effects to local collisions,¹² even where a mechanical explanation can be found, its discovery rarely occurs simply by positing well-known mechanical interactions at crucial junctures. The ideological convictions of the early mechanists led them to propose fantastic “explanations” of magnetism such as Descartes’s spiral particles (Westfall 1977, p. 143; Hesse 1970, pp. 58–59, 106, 157, 160–61; cf. Jammer 1957, pp. 105, 188, 197), which were useless because premature; sociologists may have similar temptations, but temptations to premature mechanism is not the same thing as the epistemological high ground.

Accordingly, I will admit the necessarily provisional nature of all field theories (cf. Verschuur 1993, p. 149) while arguing for their utility,¹³ and I admit that the absence of mechanisms may be a theoretical weakness in a number of respects. However, in the case of sociological analysis, there are extremely good reasons to refrain from privileging automatically a theory that can be linked to mechanisms. Because individuals (or at least individual acts) are frequently though not inevitably the level below those units described by sociological theories, mechanisms tend to involve action by individuals. While we all must appreciate the robust realism of appealing to the nature of individuals, who certainly do exist, great dangers lurk here for theorizing. This is because social science is the unique case in which the lower level appealed to by mechanistic accounts is ourselves, and we have a great number of prejudices about our own constitutions that we cannot rid ourselves of, because we do not know what all of them are. There might not have been any theory of natural selection if the Galapagos finches were the theoreticians; Galapagos finches may have very different ideas about what it means to be a Galapagos finch.

The charge of occultism must thus be turned around: for the case of the social sciences, we should not simply be suspicious of “latent” functions—we should be even more on guard regarding the “manifest” (cf.

¹² Technically, fluid mechanics is a form of continuum mechanics, where the material can be treated as continuous as opposed to composed of discrete particles, but the relation between particle mechanics and continuum mechanics is generally well understood.

¹³ As Maxwell (1954, pp. 165–66) wrote, “It must be carefully borne in mind that we have made only one step in the theory of the action of the medium. We have supposed it to be in a state of stress, but we have not in any way accounted for this stress, or explained how it is maintained. This step, however, seems to me to be an important one. . . . [But] I have not been able to make the next step, namely, to account by mechanical considerations for these stresses in the dielectric. I therefore leave the theory at this point.”

Merton 1968). It seems that people are able to be absolutely sure that they understand the reasons why they are doing something, while being—at least as far as modern neurology can tell—absolutely wrong. (For a recent and accessible discussion of some of this work, see Gazzaniga [1998].) And since social scientists and others use the word “know” to mean to bring to analytic self-consciousness, we will never correct such mistakes if we insist on basing our theories on what we all “know” to be the case about the nature of individual action.

Field theory has the notable advantage of forbidding us to apply our self-understanding wholesale, let alone to crown these prejudices with the title “mechanism” and then to congratulate ourselves on a truly *scientific* understanding. Field theory necessarily involves an *Verfremdung* in Brecht’s sense; that is, a limit to the “homeyness” of our theoretical arguments. Given the tendency of sociological theory to be the one thing that seems wholly determined by social factors, such distantiation is an important quality. This does not mean that field theory is a cure-all for projection—I shall argue below that it may bring its own distortions—but it helps prevent theoretical progress from foundering on our least alterable beliefs, beliefs about ourselves (Rokeach 1968, p. 164).

While it brings this distantiation, field theory still has a general quality of being intuitively accessible. While we cannot see magnetic fields, we can quickly come to accept that they are there, and we can understand how to navigate and manipulate them. Indeed, field theory allows us to account for the conviction Durkheim (e.g., [1902–3] 1961, p. 89) as well as many others had that there was something “more” out there—some social whole that penetrated us—without our being forced to recapitulate Durkheim’s famous difficulties in specifying exactly what this thing was.

In sum, field theory, while not without its limitations, may have signal advantages for the social sciences; I go on to discuss how field theory has been used here. Since there are a number of existing reviews of field theory (Mey 1972; Rummel 1975), I concentrate on charting out the three main directions in which field theory progressed in the social sciences: the social-psychological theory associated most notably with Lewin, the field theory of stratification or domination associated most notably with Bourdieu, and the field theory of interorganization relations associated most notably with DiMaggio and Powell. While these theories have generally been seen as rather different, and indeed come from different substantive and methodological arenas, we will see that there are fundamental affinities among the three and that all point in the same direction.

VARIETIES OF FIELD THEORY

Field theory first made its inroads into psychology with the totalistic *Gestalt* theories, which stressed that individual percepts had to be understood in relation to a wider perceptual field. Because such theories tend to be outside the domain of the social sciences, I shall not focus on them here (the main exponents were Wertheimer, Koffka, and Köhler). Such psychological approaches were intimately tied to a more fundamental and general trend in German science toward “totalistic” or “comprehensive” analysis (e.g., Koffka 1935, p. 9) that is seen in fields from critical theory to genetics (Jay 1984; Harwood 1993) and is closely linked to the philosophy of science of Cassirer (1953). Indeed, a number of “applications” of “field theory” in the social sciences are little more than pronouncements that the researcher should see “the whole story”; such accounts are not examined here.¹⁴ But Kurt Lewin’s adoption of this totalistic perspective into social psychology brought field theory into a position where it was relevant for the social sciences and had implications for theorizing; I begin with this approach.

Social-Psychological Fields

Gestalt theorists had argued that (contra the atomistic approach of many behaviorists), one could not understand how an organism sensed the environment without attention to the field of perception as a whole.¹⁵ Any one percept [bit of perception] was likely to have its meaning only in relation to others. Thus Köhler recalled that his goal was to determine “why percepts at a distance have an effect on one another. This is only possible, we assumed (and we followed Faraday in doing so), if the individual percept has a field and if the ‘field,’ which surrounds the percept, does not merely reveal the presence of this percept but also presents its specific properties” (cited in Mey 1972, pp. 13–15; for a discussion of the relation of *Gestalt* theory to field theory, see Mohr [in press]). Tolman and

¹⁴ The first serious introduction of field theory to the social sciences other than social psychology was Karl Brandt’s (1952) use in economics (though also see Geiger [1949, pp. 51–52]). While he made few actual theoretical contributions, Brandt’s understanding of field theory was excellent and so he will be referred to as a field theorist though not reviewed. Even earlier, Lundberg (1939, esp. pp. 103, 260, 311) had, drawing upon *Gestalt* theory, incorporated aspects of field theory into his system, but they were merely one minor part of a conglomerate theory that lacked simple coherence (though many aspects are still impressive today).

¹⁵ While they adopted the idea of “field” from the visual field, it is interesting to note that Maxwell (1954, p. ix) also saw field theory as wholistic—he commented that “Faraday’s methods resembled those in which we begin with the whole and arrive at the parts by analysis, while the ordinary mathematical methods were founded on the principle of beginning with the parts and building up the whole by synthesis.”

Brunswik (1935) went farther and argued that perception had to be understood not as the passive internalization of sensation but as the organism's attempt to navigate a world that had its own "causal texture"; hence the trick was to "get" the principles that would allow for effective action. This suggested to Tolman that animals should have the ability to orient themselves to a complete spatial whole when learning a path as opposed to simply memorizing a set of reinforced actions such as turns, something he demonstrated with rats (Tolman, Ritchie, and Kalish 1946a, 1946b), though his work was ridiculed and ignored by a generation of orthodox behaviorists (see Gould and Gould 1999, p. 67). The orthodox behaviorist model, as Rummel (1975, p. 25) and Köhler (1947, pp. 106, 121) point out, is comparable to the mechanistic interpretation of action at a distance in contrast to a field one: without an explicable chain of elements banging in to one another, the phenomenon had to be wrong.

Tolman (1954) went on to contribute a field theory of psychology for the Parsons and Shils volume *Toward a General Theory of Action*. As this effort, reasonable though it was in itself, had little to do with field theory, other than its Lewinian proliferation of drawings that seemed somewhat in between plans for a football play and plans for a transistor radio, I do not consider it further here. This field approach to perception was taken over by J. J. Gibson (e.g., 1986), whose work is an important example of field theory, though somewhat outside the social sciences.

Instead it was Lewin, a colleague of Köhler and Wertheimer at the Psychological Institute (see Marrow 1969, p. 13), who put field theory on the map in social psychology. While Lewin (1951, p. 240) claimed to find his inspiration for his conception of field in Einstein, his definition of a field as "a totality of coexisting facts which are conceived of as mutually dependent" is clearly derived from the *Gestalt* emphasis on totality. In particular, he famously argued, behavior should be defined as a function of both personality and environment, with the added complications that environment is a function of personality, and personality a function of environment.

Such statements of total generality of course strike today's social scientist as utterly pointless, but they evidently served to free many psychological theorists from the need to consider each organism in theoretical isolation. Lewin, however, was not content simply to stay at this level of abstraction: he also alternated between impressionistic and theoretically ambiguous topographical drawings to show what he was getting at and overly precise mathematical formalizations. His few empirical examples cannot be taken seriously in retrospect (cf. Eng 1978), and his later use of field theory—elaborate multishaded topological maps showing how, say, autocracy narrowed the space of free choice among group members—

were, even if correct, no more scientifically persuasive than a model made of toothpicks and marshmallows over dessert.

Yet there was something theoretically important underlying even the seemingly vacuous formalizations linking behavior to personality and the environment, though it was often obscured by the premature formalization. Orthodox behaviorism worked and still works very well on its own turf: on animals strapped down and exposed to a set of distinct stimuli (see also Leontyev 1977, p. 180). But the mobile animal cannot be passively exposed to the same stimuli; it nears the desired object (when it is safe) and evades the feared. A behaviorist analysis of the mobile animal—hundreds of stimuli and responses for each movement of a few inches—would appear as absurd as a blow-by-blow description of a large battle. Lewin’s terminology comes into its own in such a situation.

The animal (or person) is, first of all, in a phenomenological life world: that is, the world as it appears to him or her. Lewin then made three crucial contributions. First, the life world is, according to Lewin, intrinsically *affective*—in contrast to stimuli considered solely as stimuli (light stimulates retinal cells), these phenomena are perceived immediately as desirable or undesirable. These *Aufforderungscharaktere* (usually translated “valences”)¹⁶ determines how objects and other beings construct the field.

Second, the animal (or person) is free to move about in the field (cf. Koffka 1935, p. 384). Because of his topographic imagery, Lewin frequently confounded Cartesian space with the space of the field. But this confounding is in the world, not in Lewin’s theory, for movement in the field (as he described it) did in fact frequently correlate with movement in space. Since we all move through time at the same rate of one hour per hour, we tend to ignore purely temporal movement when understanding our own actions (with the exception of “waiting,” itself a surprisingly rare strategy, though one highlighted by Bourdieu 1988). The rat sees the maze as a series of obstacles standing between himself and the desired cheese; progress in the space of the field corresponds roughly to geometrical progress toward the cheese. Indeed, Lewin was sufficiently sensitive to the importance of the difference between field and Cartesian space to focus on experiments that precisely tested which subjects (animals and children) could themselves make this dissociation and move “away” in geometrical space from a desired object in order to approach it in the

¹⁶ While “valence” is the standard translation, Allport (1955) and Koffka (1935, p. 35) used “demand character” and Brown “invitational character” (Marrow 1969, pp. 56–57, 61).

field space, as in classic studies by Köhler (also cf. Koffka 1935, p. 275).¹⁷ Movement thus needs to be analyzed not in terms of locomotion through physical space but as directed action in the field—an “aim path” of striving (see Mey 1972, p. 18).

Third, the animal (or person) has conceptions of likely *changes* in the field at any time. These changes are produced both by the animal’s own motion through the field and by internal developments of the field itself, which may or may not involve actions taken by other animals in the field. This has two implications. The first is minor but vexing: at least one additional dimension (the subject’s conception of the future and past states of the field) must be added to the already busy diagrams. While Lewin figured out reasonable ways of accomplishing this, there was a practical problem of how to use paper to represent increasingly higher dimensional figures.

A more important implication—and one that, interpreted consistently, could solve the former problem—is that the past cannot directly affect the present. In contrast to what he considered primitive views of causality, Lewin (1936, p. 10) argued that behavior should not be seen as caused by something in the past (let alone the future), but must be grounded in an understanding of the totality of the current situation. (We may then ask how this situation came into being, a historical question quite different from the systematic question posed by field theory [Lewin 1936, pp. 30–31, 34].) This “principle of ‘contemporaneity’” flows directly from the fundamentals of field theory (Lewin 1936, pp. 33, 35; cf. Koffka 1935, p. 429).

Here field theory correctly understood made an interesting and reasonable contribution to social analysis that cut against dominant approaches. But Lewin’s use of field theory brought at least as many problems as it solved. For one thing, Lewin attempted to combine a metric notion of field taken from physics with a wholly distance-less understanding from topology—two approaches that are fundamentally inconsistent (Rummel 1975, pp. 43, 38, 41; Spiegel 1961, p. 17; though see Mey [1972, p. 40] for a defense). Lewin (1936, pp. 53, 55, 85) later realized the tension between the two approaches and suggested a division of labor: topological analysis would determine the possibility or impossibility of certain trajectories, while vector analysis would determine their relative likelihood. In practice, he generally favored the topological approach for substantive problems because he might then sketch any particular claim he was making, but he switched to the metric notion of field to make wholly general

¹⁷ A child (or ape) is placed inside a U-shaped set of three walls, able to see something desired on the other side of the middle wall. To get to the desired object, she must first walk *away* from the object and then go around the side wall.

(and largely meaningless) mathematical formalizations involving differential equations (Lewin 1951).

Even more important, there were severe limitations built into Lewin's definitions, especially insofar as he tried to make the field wholly psychological. Most important were the limitations in his conception of valences. A valence is something that pulls one toward or pushes one away: the field itself may be seen as the product of many valences, as a gravitational field may be seen as the product of many objects each with its own gravitational field.

This seemingly unremarkable definition, however, leads to paradox, because Lewin considered the valence to be "in the head" of any person in question. Accordingly, any need, desire, or drive held by the person or animal itself has a valence. It then becomes not the cheese that has the valence, but the hunger of the rat. The field continually collapses to a point; Lewin is in the position of someone holding one end of a string, and forced to argue that the pull he feels comes not from the other end, but his own end. Similarly, Lewin understood the field to consist of everything relevant to the person in question at one time: "What is real is what has effects." But since actors do not always know about all the factors that are in fact relevant, Lewin (1936, p. 19) was forced to conclude that the psychological life space—the field which he claimed to be in the head of the acting subject—contains elements that are wholly outside this person's psyche.

Resolution clearly requires a sense of a social, as opposed to psychological, field (cf. Simonis 1974, pp. 368, 372); or at least a transpersonal or geographic field as in Koffka (1935, pp. 63, 345, 357, 376, 664, 675). Lewin did sometimes speak of a social space or social field, by which he meant the joint life space of more than one person (Mey 1972, pp. 61–64): unfortunately, joining two or more unworkable topological models did not increase the concreteness or usefulness of his scheme. In fact, his approach made it difficult to understand why the life spaces of two people would have anything to do with one another.

Lewin's theory was enormously influential among a moderate number of social psychologists, who were attracted by the comprehensive nature of the philosophy, the personal charisma of certain believers, the promise of formalization, or the social activism underneath. But there were few contributions after Lewin. The most noteworthy work, by J. F. Brown (1936*a*) (often miscited as B. F. Brown), is justly if cruelly summarized by Rummel (1975, p. 54) as "low grade sociology and political science, tendentious comments on the sins of capitalism and the virtues of Marxist socialism, and the most naïve observations on Soviet communism—all sprinkled with obtrusive 'barriers,' 'regions,' 'locomotion,' and 'vectors.'" The lack of progress seems to have come from the difficulty of going

further with a fundamentally psychological understanding of the field and of valences. It was necessary to link the pseudospacial organization of demand-characteristics (valences) to something *interpersonal* if this spatial logic was to be anything other than tautology. This came with the importation of field theory into sociology proper.

Fields of Organized Striving

Here we must backtrack briefly, as field theory was most successful when joined to a preexisting line of inquiry. This line essentially stemmed from Max Weber's sociologization of the idea of "spheres of value." Around this time in the circle around Weber there was a general emphasis on the ethical dilemmas that arose because of the necessary conflict between spheres with their own "inner laws" (see Burger 1976, p. 8; Goldman 1988, p. 136; Schluchter 1996, p. 278, n. 18; and Habermas 1996, p. 409; Mannheim [1940, pp. 159–60], in the work in which he proposed a field approach, also discussed Weber's value spheres). But it was Heinrich Rickert (1913) who formalized this and logically derived six types of values, which Weber adopted for his key theoretical piece, "Religious Rejections of the World and Their Directions" (Weber [1915] 1946), though Weber's six spheres were slightly different from Rickert's. Weber emphasized the "inherent lawfulness" (*Eigengesetzlichkeit*) of each of these spheres that led toward a purification or rationalization of purpose and consequent tension between spheres, as one could not "serve two gods" (e.g., religion and science) at the same time.

There was something deeply compelling about this basic vision, but also something fundamentally asociological—following Rickert, Weber had simply declared that there were six value spheres and given very little justification for this. The *Gestalt* tradition in general and field theory in particular gave a number of German social scientists the tools to come up with an (in principle) empirically grounded approach to these value spheres. (While Pierre Bourdieu became the most prominent exponent of such a field theory, this approach was first developed in Germany.) In this light, value spheres exist not because of the transcendent nature of human action, but because of the existence of some social logic to the social goals held by actors. In the words of Victor Turner (1974, p. 135), the field is "an ensemble of relationships between actors antagonistically oriented to the same prizes or values." Accordingly, I will call this branch of field theory a conception of "fields of organized striving." It was here that field theory was first seriously applied to the social sciences (e.g., Brandt 1952, p. 188).

The first notable effort in this direction was Friedrich Fürstenberg's (1969) analysis of the process of upward social mobility or social ascent

(*Das Aufstiegsprozess*) using a mixture of field theoretical concepts and closure theory.¹⁸ I will focus on his theory, highlighting similarities to the better known approach of Bourdieu, and then I will discuss further refinements made by Bourdieu and others. First of all, instead of speaking of spheres of value as did Weber, Fürstenberg analyzes “sectors of ascension” (*Aufstiegsektoren*). His list of sectors, however, is close to Weber’s, including economics, politics, bureaucracy, religion, and intellectual sectors, as well as a few others (Fürstenberg 1969, pp. 67–69).

Like Weber, then, Fürstenberg sees there being necessary divergences due to choices between our goals. But unlike Weber, Fürstenberg—influenced by comprehensive approaches—assumes that these sectors cannot be analyzed independently, since all are part of the same social field (and here he cites Lewin). Fürstenberg (1969, pp. 51, 122) chose the field analogy in part because it emphasized that the result of any individual’s action was due to the interaction between the state of the field and the states of the individual. The social climbing process can thus be seen as a “chain of interrelationships between the ascending individual and the current social environment” (Fürstenberg 1969, p. 52).

Consequently, like Bourdieu, Fürstenberg (1969, pp. 70–71, 51, 74) sees trajectory as a crucial aspect of the navigation of fields; he speaks of the safe but limited path of the career track (*Laufbahn*) that has institutionally defined boundaries as the limiting example of the tendency for the field to give the individual a “social fate.” But there are other strategies and trajectories of mobility (e.g., marrying into a higher group). Accordingly, like Bourdieu, Fürstenberg (1969, p. 42) stresses that our techniques of analyzing movement in the social structure have to be as multidimensional as their object.

Perhaps most important, Fürstenberg (1969, pp. 36, 37, 42, 49, 54–55), like Bourdieu, attempts to combine objective structural analysis and a positional subjective analysis. On the one hand, the influence of objective social position is necessarily mediated by subjective perception, and the objective structural trajectory must have a “subjective correlate” in an individual striving for success. In particular, Fürstenberg (1969, p. 159) attributes to each person a subjective “aspiration level” (*Anspruchsniveau*).

¹⁸ Spiegel (1961) put forward a field theoretic analysis of opinion change in which a field formed around the introduction of an object of opinion (*Meinungsgegenstand*) such as a consumer article, with people dividing into adherents and abstainers depending on the location of the object in a space of preferences; this interesting approach has something of the notion of the field of striving, but as it remained somewhat unique (until the development of the ecological theory of markets) it is not reviewed separately.

veau),¹⁹ consisting of expectations, in addition to an objective aspiration level corresponding to the demands (*Anforderung*) of the individual's social environment. The former tends to reflect the influences of parents, peer influence, school, mass media, and later, the work world.

On the other hand, as the issue of objective aspiration levels suggests, Fürstenberg (1969, pp. 142–43) believes that subjectivity will tend to correspond to objective position, though he also makes allowance for unrealistic hopes. Somewhat like Bourdieu, Fürstenberg (1969, pp. 148, 59, 80–81) stresses the importance of value socialization—especially the internalization of the norm system of the school—for future members of the elite. Indeed, he argues that this internalization, and not differential ability, explains the correlation between educational attainment and social status. “The social stratum thus influences the evolution of the subjective aspiration level and with this also the academic actualization of the existing potential ability.”

Even more important is the correspondence between objective position and subjectivity that comes from the demands placed upon the individual in the field. To explicate this, Fürstenberg (1969, pp. 53, 102, 121–22) argues that a field necessarily has specific roles even if it does not have organizations in the usual sense, because there are differentiated action imperatives that he considers “role-demands” (*Rollenanforderungen*).

But the use of role theory implies a potential slippage between these demands and the person. Merton (1957) had argued that there were actually a set of different expectations for any status (the “role set”) and these expectations could come into conflict. Similarly, Fürstenberg (1969, p. 63) assumes that these role expectations are equivalent to the field effect: the situation is experienced by the individual as a chain of objective requirements (*objektiver Anforderung*)—whether these are formal or informal is of secondary importance. This suggests a new approach to role. While in the classic definition of Linton (1936) the role is basically the carrying out of a status, itself a collection of definable rights and duties equivalent to a qualitative slot in social structure, the field theoretic conception suggests that role may more generally be defined as the subjective counterpart to a less clearly defined (but no less real or important) continuously variable position in a field (also see Lundberg 1939, p. 312). Further, as we shall see below, this implies that advances in role theory associated with network analysis may help better define what is meant by the “field.”

This line of field theory has been brought to a more finished state by

¹⁹ This term was first coined by a student of Lewin's (Tamara Dembo) and then investigated by another student, Ferdinand Hoppe (Marrow 1969, pp. 44, 56; Fürstenberg 1969, p. 52, n. 2).

Pierre Bourdieu. While Bourdieu in his early work made references to Lewin (see Swartz 1997, p. 123, n. 15), he generally does not highlight the connections of his approach to other field theorists; however the overlap suffices to allow us to focus on those aspects of his approach to fields that are distinctive and improve on previous usage. I then point to several areas where other field theoretic approaches may contribute to Bourdieu's work.

Bourdieu (e.g., 1985*b*, p. 723) sees field theory as a social topology, but one that is differentiated into several domains that, while connected in the substratum of the same social space, may be treated as analytically distinct (though the degree of autonomy of any field is of course an empirical question). These fields have changed over the course of his work: at some times he emphasizes an overall social field reducible to dimensions of capital, economic, cultural, social and symbolic (Bourdieu 1985*b*, p. 724); more generally (e.g., Bourdieu 1984) fields coincide with familiar divisions of action into self-contained realms of endeavor such as sport or art. Bourdieu himself has analyzed a number of fields: that of photography, that of literature, that of the French academic world.

Each agent is defined by her position in a field with its own themes and problems, at least so far as the field possesses autonomy (Bourdieu 1969, pp. 161–62). Accordingly, each field has an *Eigengesetzlichkeit* (cf. Bourdieu 1990*b*, p. 389; 1993, p. 72). This does not mean that external events or factors are not important for actors, but they do need to be translated to the internal logic of the field (see Swartz 1997, pp. 128, 215)—akin to the principle that the magnet may cause the field, but it is the field that has the effects on the iron filings.

Every particular field has a coherence based on a working consensus as to the nature of the game, and people take predictable sides due to the more general structuring of social space. Yet this coherence is a dynamic one, for “every field is the site of a more or less overt struggle over the definition of the legitimate principles of the division of the field” (Bourdieu 1985*b*, p. 734). What is at stake in a chess, tennis, or sumo tournament is not simply which individual will be the winner, but what *kind* of chess, tennis, or sumo (and hence, what *kinds* of players) will dominate the field in the future.

Bourdieu goes beyond Fürstenberg in adding that striving in the fields is coordinated neither by “ideology” nor by conscious strategy but by the habitus, a cultural unconscious, a matrix of dispositions that serves to affectively organize perceptions (Bourdieu 1969, p. 182).²⁰ Most important,

²⁰ It is often incorrectly assumed that Bourdieu more or less invented this word; he himself cites Panofsky as his influence, but the word is simply the Latin for habit and was given special philosophical meanings by William of Ockham (see Fuchs 1952;

habitus is linked to field position (or at least position in social space, in turn related to field position). This leads to an “ontological complicity” between the world and our faculties for making sense of it.

Sociological critics have seized upon this as supposedly doing insufficient justice to the potential of actors to transform the structures that gave rise to their habitus, which seems a rather superficial criticism, since it turns on whether there is “enough” or “not enough” emphasis on change—degree of emphasis being notoriously hard to measure. Complaints about emphasis aside, it can be seen that Bourdieu’s conception is rigorously in line with the fundamental postulate made by Tolman and Brunswik (1935), namely that perception is organized in terms of making sense of a prestructured causal world. Indeed, it is difficult to imagine how this could not, in some form, be the case.

Thus Bourdieu (1988, p. 101), like Fürstenberg, highlights the importance of social fate—“the dialectic of consecration which helps to propel agents toward the places to which their socially constituted dispositions predestine them.” Accusing this understanding of giving too much “emphasis” to conformists is not all that different from criticizing a psychological theory of perception for giving too much “emphasis” to the principle that perceptions occupying a smaller proportion of the visual field are likely to be farther away. This built-in correction may not always be correct, but it is a basic part of our perceptual system, and a theory of perception must recognize it. So too it is reasonable for Bourdieu to propose a basic cognitive system that builds upon regularities in the causal structure of the environment without thereby being taken to make a claim that these regularities must persist.

Others have criticized the proliferation of fields in Bourdieu’s theory and the uncertainty that necessarily arises when the boundaries and object of the field are objects of struggle (e.g., Swartz 1997, pp. 122, 132); I believe this is mistaken. Field theory is an analytic approach, not a static formal system. If indeed a university professor of philosophy in France in 1968 is oriented to an interdisciplinary intellectual field, and the result of struggle over the proper role of the intellectual leads this same person to be oriented to a disciplinary philosophical and academic field in 1985, field theory should be able to analyze this change, not have the categorical rigidity that will prevent it from so doing.

Thus Bourdieu builds upon the basic field theoretic approach to give an intuitively appealing and theoretically generative account of the sociology of striving. In addition, he alone builds a *sociologically* (as opposed

Bourdieu 1985a; 1993, p. 86). Interestingly, Ockham was the first Western scholar to renounce the scholastic principle of “no motion without contact” and explicitly allow for action at a distance (Jammer 1957, p. 64).

to ethically) compelling version of reflexivity into his analysis. Bourdieu accepts the basic claims of what we can call “perspectivism”—namely that even observations of the social world made in good faith will bear traces of the observer’s own position in this world. In contrast to others who imagined reflexivity to involve merely the sociological hero baring his or her (invariably noble) value commitments, Bourdieu’s understanding requires the analyst to correctly position him- or herself in the field of relations between other analysts and other actors. This allows him to go beyond Mannheim’s (1936) reasonable if simplistic argument that correct knowledge requires the multiplicity of views, by positing that what we need is less the points of view themselves, than an overall understanding of the relative placement of these points. “The scientific virtue . . . of the notion of the field resides no doubt in the fact that this notion tends to exclude those partial and unilateral objectifications of the unconscious of other people” (Bourdieu 1988, pp. xvi, 11). As Mead says, “A perspective can be recognized as such only when lying in a field within which it is no longer a perspective” (1938, pp. 64, 606–7).

This sober notion of reflexivity—and its fundamental integration with the field theory of striving—represents an important advance in field theory. However, Bourdieu’s attempt to explain his approach has often led to unnecessary confusions and mistaken critiques. Most importantly, in emphasizing the futility of the opposition between subjective and objective approaches to sociological explanation, Bourdieu (e.g., 1988, pp. xiv, 18) frequently retains the use of traditional (if somewhat loose) labels and implies that these are two moments of sociological endeavor that are basically sound but partial, and need only to be combined. On the one hand, there is the “subjective” analysis that Bourdieu often calls “constructivism,” namely the notion that, to put it crudely, “it’s all in the mind.” On the other hand is “objectivism,” which Bourdieu frequently identifies with “structuralism.” But Bourdieu clearly does not mean to subscribe to the “both/and” resolution spoken of earlier. This is clearest in his treatment of agency, which he explicitly does not see as a voluntaristic residue in opposition to structure (e.g., Bourdieu 1988, pp. 149–50). Yet readers are often left with the impression of a two-pronged analysis of objectivism and subjectivism, as opposed to the conceptual unification he intends.

It may be that this comes in part from Bourdieu’s tendency to give minimal attention to organizations (noted by Swartz 1997, p. 215). Without the specificity afforded by attention to concrete organizational forms, the field is only seen in subjective dispositions, and hence there is a tendency to oscillate between subjectivist constructions and structural analysis of the largest kind. Bourdieu argues that the ambiguity in the relation between institutions and fields in his work is intentional (see Bourdieu and

Wacquant 1992, p. 232). But it may be that his approach is completed, not contravened, by a restriction of field to interinstitutional relations. This assumption guided the third branch of field theory in the social sciences, to which we now turn.

Institutional Fields

The final conception of field was first suggested briefly by Mannheim (1940, pp. 295–98) to describe the case of interdependent actions that transcend organizations or groups (“sector fields”), his example being international merchants. A field structure develops when units interact in such a way that they develop a mutual influence irreducible to existing institutional channels; the most important case is when they align because they are pursuing similar ends (cf. Cooley 1913, p. 553). When “conflict and competition are in full swing, and individuals have to make their own adjustment” without recourse to concrete groups, Mannheim argues, a field structure will tend to arise. This can be best understood, Mannheim says, by comparison to a magnetic field, and in such cases, the force of this field will overpower those of “established custom or rational organization.”

This idea of Mannheim’s was picked up by Ronald Warren (1967, p. 397), who also explicitly claimed to be using the word “field” in Lewin’s sense. (Warren’s adoption of field terminology has been recently discussed by Mohr [in press].) Warren focused on the field as something that explained transorganizational consistencies. It is easy to explain how an organization structures actions—organizations have rules, goals, rewards, and sanctions. It was less easy to explain how organizations themselves were structured.

In addition to Mannheim and Lewin, Warren also drew on important work by Emery and Trist (1965), who also acknowledged the influence of *Gestalt* theory and Lewin (Emery and Trist 1965, pp. 21, 28). They began with an interest in the organization’s environment and drew upon the *Gestalt* theorists’ idea of the field as a field of total perception. Starting from Tolman and Brunswik (1935), they argued that the organization’s actions had to be understood as correlative to the degree of structure of its environment. In a simple, random field, the organization—like an organism in an undifferentiated environment—may as well be one place as any other. “There is no distinction between tactics and strategy” (Emery and Trist 1965, p. 24), since all action is local, and one attempts to do as well as one can without needing to plan a trajectory (link local action to global pattern).

More structure is present when the environment is “clustered” (e.g., a niche market)—here organizations will diversify and move to different

locations of the space, perhaps, just as animals will move to where the food is. Even more structure arises with the presence of similar organizations of the same kind, which must be taken into account by each organization when planning its strategy. Now action takes on the characteristic of a game in the technical sense. The final level of structure they propose is the “turbulent field” in which dynamics arise from “the field itself. The ‘ground is in motion’” (Emery and Trist 1965, p. 26; cf. Scott and Meyer 1992, p. 142). Emery and Trist are not quite clear as to what this means, but they suggest that this situation arises from the growing interdependence of different organizations and increasing competition and strategic vision; certainly it will involve the “positive correlation” of the fates of different organizations, which they consider the “organizational matrix” (Emery and Trist 1965, p. 29). And now the goal of action becomes “institutionalization”—a stable position in the field; no longer are goals defined independently of the overall field.

This line of theorizing was brought to its current state of completion largely by DiMaggio and Powell (1983), who tied this perspective to that of Bourdieu.²¹ They begin with a somewhat different question from Warren, namely why do organizations look so much alike? To answer this question, DiMaggio and Powell define the organizational field as “those organizations that, in the aggregate, constitute a recognized area of institutional life: key suppliers, resource and product consumers, regulatory agencies, and other organizations that produce similar services or products.” They argue that fields develop when there is an increase in extent of interaction among organizations in the field; the emergence of sharply defined interorganizational structures of domination and patterns of coalition; an increase in the information load with which organizations in a field must contend; and the development of mutual awareness among participants in a set of organizations that they are involved in a common enterprise.

Most important, and in contrast to most field theorists (including Emery and Trist), DiMaggio and Powell went beyond simply suggesting that a field caused the elements within it to align in some way, or drawing pictures of such alignment. Instead, they suggested how this alignment may occur and hence how it can be concretely examined. In particular, they suggest that the structuration of the field is largely a result of patterns of relations; drawing on White, Boorman, and Breiger (1976) we may say they are relations of both direct interaction and of structural equivalence in ties. Two different—even hostile—firms may be drawn closer together because they share suppliers and distributors. This conception of “role”

²¹ Mohr (in press, n. 11) notes that the original version of the DiMaggio and Powell piece cites both Bourdieu and Warren as inspirations.

as a global patterning to ties (coming from Nadel 1957) may better specify the intuition that Fürstenberg had that the field was, in a sense, composed of roles. This suggests that field position can empirically be specified by a close study of the ensemble of interpersonal relations; indeed, that the field effect involves a reduction of these myriad relationships to a manageable position, in the same way that a role simplifies many interactions with different concrete others (cf. Bourdieu and Wacquant 1992, pp. 113–14).

I will close by suggesting that this focus on patterns of relationships may be profitable for the more general case of transinstitutional (and not simply transorganizational) structure. This would suggest that the field theories we have examined are not merely tied via common descent and shared metaphors, but are special cases of a more general (although consequently open-ended) field theory. I first highlight the distinctive features of field theory that can be found across these three traditions; I then draw them together.

COMMONALITIES IN FIELD THEORY

We have seen three different approaches to field theory; the practitioners of each, however, have indicated their faith in underlying connections (here see Lewin 1999*b*, pp. 30, 32; Bourdieu and Wacquant 1992, p. 97; and Swartz 1997, p. 123). But the commonalities are more than genetic: field theorists generally share conceptions of what a field is, how analysis should be conducted, how causality should be interpreted, and most important, how we understand the relation of fields to individuals and their cognition. Of course, there are elements of disagreement, if only because all the theorists were influenced by more than one theoretical tradition. In ignoring other aspects of the above theories, I do not mean to claim that these divergences were of no consequence. But I do claim that the areas of convergence, far from being merely some accidental overlap, indicate the nature of field theory as a general explanatory approach for the social sciences.

Three Senses of Field

First of all, there is a tendency for field theorists to use the word “field” in three overlapping or interrelated senses. In the first, there is the purely topological sense emphasized by Lewin: the field is conceived as an analytic area of simplified dimensions in which we position persons or institutions. Second, there is the sense of a field as an organization of forces. Third, there is the sense of the field as a field of contestation, a battlefield.

Regarding the first of these senses, the general topological understanding must be admitted to add nothing in and of itself. The proof must be in the pudding; while logically field theory may suggest that any field must be infinitely complex with infinite dimensions, in practice a useful field theory must specify a small number of dimensions (or a nondimensional but finitely structured space) in order to have utility. However, a field theory is not simply a spatial model—while a field is, as Bourdieu (1993, p. 72) says, a structured set of positions, and positions can often be understood in spatial terms (see Brown 1936*a*, p. 476), not all sets of relative positions can be understood as a conventional space (since “distances” may not work according to spatial logic; see Brown 1936*b*, pp.79–80). Indeed, this is what is established by the Köhler-type studies that distinguish “as the crow flies” distance from phenomenological distance. Further, not all spatial models imply a field: a field is different from a “Blau space” (as McPherson, Popielarz, and Drobnic 1992 call it) in which the dimensions are individual *attributes*. The position of persons in a field must be based on their interpersonal relations (cf. Lundberg 1939, pp. 104, 263), or on their orientations to each other or to shared goals: whether a set of persons actually forms a field must be an empirical question, and cannot be true by definition or methodology.

Regarding the second of these senses, with its overtones from physics, one must acknowledge that social scientists should be extremely wary of sloppy importations from other disciplines, especially those of the most prestigious natural sciences.²² Having been too frequently burned by such panaceas, we must bear in mind that something useful in one science may be worthless for another. However, field theorists agree as to the advantages of this conception of field in not only leading to intrinsically relational thinking, but in the *Verfremdung* spoken of above. As Bourdieu (1990*a*, p. 192; see also Bourdieu 1993, p. 21) says, “To think in terms of field demands a conversion of the whole ordinary vision of the social world which fastens only on visible things [i.e., the individual and the group]. . . . In fact, just as the Newtonian theory of gravitation could only be constructed against Cartesian realism which wanted to recognize no mode of action other than collision, direct contact, the notion of field presupposes a break with the realist representation which leads us to reduce the effect of the *environment* to the effect of direct action as actualized during an interaction.”²³

The first sense of field came from field theory’s ultimate origins in the

²² Bourdieu has both compared social fields to magnetic fields (e.g., 1969, p. 161) and castigated those who do so (1988, p. 149).

²³ Here I substitute the translation of Wacquant in Bourdieu and Wacquant (1992, p. 96n48).

psychology of perception (Lewin 1936, p. 14), while the second came from the analogy to physics. The third sense—a field of contestation—may initially seem somewhat at odds with the others. But while Bourdieu more than others has stressed this third meaning of the word *field*, he is not unique in this regard (see, e.g., Turner 1974, p. 42; Mannheim 1940, p. 298; Mey 1972, p. xv). In particular, Lewin, who rarely discussed social conflict as such, may still have had the same martial images in the back of his head that one sees in Bourdieu: one of his early papers (Lewin 1917) was entitled “Battlefield” and was a striking phenomenological analysis of the landscape of conflict. The battlefield experiences described here may have been decisive for the development of Lewin’s theory (Rummel 1975, p. 35; Eng 1978; Marrow 1969, p. 11; Mey 1972, p. 40).

Given this understanding of the field as a field of self-organized contestation, one might hope that such conflict can provide the mechanism that makes the idea of the field as a field of force unnecessary.²⁴ Indeed, it does seem reasonable to imagine that field effects should, in some sense, be ultimately reducible to the organization of goal-directed striving (which can of course involve alliance or independence as well as conflict). While such an account is not necessarily empty, it is, however, necessarily incomplete. First of all, it is one thing to express confidence that some aggregate effect can, in principle, be reduced to smaller interactions and another thing to find explanatory purchase in such reduction. But more important, this sense of the field as goal-oriented striving cannot replace the other conceptions of the field, since to do so begs the question of what it is the actors are striving for. Field theory argues that this must be understood as endogenous to the field, as opposed to wholly exogenous (Brown 1936a, p. 272).

Thus while we may see action in the field as in some sense composed of often conflicting strivings, we cannot dispense with the central idea of the field as that which induces motion. Wedding these two conceptions in a theoretically consistent way, however, can prove difficult. Lewin occasionally suggested that conflict arises from opposing but overlapping fields of force, which (as Mey [1972, pp. 43–44] points out) does not make technical sense: at any point there is, by definition, one and only one vector in a field. Lewin’s student Brown (1936a, pp. 55–56), following his teacher’s topological model, tended to emphasize boundaries when dealing with conflict, thereby completely inverting the nature of the field from that which induces motion to that which impedes it. Köhler (1947, pp. 131–32) built upon the technical idea of “stress” in a medium (an idea from fluid mechanics that served as the analogical basis for the development of the tensor equations of field theory) to propose a tendency

²⁴ I thank a reviewer for stressing this.

toward “dynamic self-distribution,” but this does not help us understand how this process may occur in the social realm. The most promising way of understanding the field’s ability to provide goals while being a site of conflict seems to be along the lines of considering the field as a “game” with rules. This sense that social life can be compared to a game in the familiar sense (and not the game-theoretic sense) may offer a partial, though incomplete, way of understanding how these three senses of field might refer to the same phenomena.

A game involves struggle between persons to reach some goal or goals, but as Mannheim (1940, p. 298) says, the presence of struggle “does not prevent the game being played according to certain rules.” Social fields differ from board games, however, in that the struggle is both over and within the rules, and thus the “game” is not some sort of overarching formal framework that in some obscure way *forces* persons to do this or that (see Turner 1994, esp. p. 115). Köhler (1947, p. 131) clarifies the difference by contrasting the Aristotelian conception of the motion of the planets with the Newtonian. In the former, the regularity of motion had to come from some external constraint—crystal spheres *forced* the planets to stay in their paths. In the latter, it is the “free play of gravitational vectors” that leads to the orderly arrangements of motion that we see as due to the “law” of gravity.

There is, of course, something odd in the transference of the idea of “law” from the human realm to the natural realm and something doubly odd in bringing this altered conception of law back to the human realm in the form of social laws that supposedly were made by no one but determined all. The same tension between rules as prescriptions and rules as descriptions is found, in attenuated form, in many uses of the game metaphor. The understanding of fields as the result of alignments between actors oriented to related prizes clarifies the status of rules: they are neither unalterable constraints that channel our action or make “illegal” action unthinkable nor mere descriptions of central tendency. When patterns of conduct are recognized by actors as forms of regularity, conformity or nonconformity to the pattern, whatever advantages or disadvantages may also follow, has semiotic import (here see Swidler 1986, 2001). Like a poet breaking meter for emphasis, players break the rules precisely because they are rules.

Accordingly, field theory is well equipped to deal with one of the fundamental weaknesses of mainstream sociological theory, namely its inability to do much with cases in which persons stand somewhat apart from the patterns of regularity upon which sociology focuses. Mainstream sociological theory, which considers the “socialized” person to have preferences, needs, and desires that are “socially formed,” generally has a difficult time explaining why people have such distantiation from “their”

own values. Parsons and Shils (1954) tried to explain this as a result of “faulty internalization,” which can produce the “derivative” need-dispositions “to refuse to fulfill expectations.” This explanation thereby asymmetrically assumed that fulfilling expectations could never be a result of “faulty” internalization. Only Goffman (1959) had an orientation to norms that came anywhere near to capturing the obvious truths that conformity or nonconformity are strategic options with different advantages.

The emphasis on the agonistic struggle of the “game,” then, has the important advantage of focusing attention on how regularity, far from being assumed to be a “causal” structure of forces which will compel all future persons to act similarly, is a set of assumptions always vulnerable to deliberate upset via surprise. For an example, I take what I believe to be the first recorded case in history (371 B.C.E.) in which a social “law”—one that was understood by contemporaries to be explained in terms of individual-level mechanisms—was deliberately undone precisely *because* it was a law. Greek phalanx (or hoplite) warfare involved the confrontation of two lines of soldiers, each armed with a shield on the left arm and a spear or sword in the right. These two lines would slowly rotate in a counterclockwise direction, and the “microlevel” mechanisms of this regular pattern are explained by Thucydides (V:71): each soldier’s right side was protected only by the shield of the man to his right.²⁵ As each instinctively moved closer to the right, the line shifted; to keep the lines aligned, the right flanks would move forward.²⁶ Captains, aware of this “law,” tended to put the most valiant fighters on the right side, as they would lead the aggression into the enemy’s side. But the most interesting thing about this “law,” of course, was that it was not a law at all, though it was repeated for generations. Then the Thebans, though vastly outnumbered, won a major battle over Sparta at Luctra by concentrating their forces on the left instead of the right and breaking through this side of the Spartan phalanx (Keegan 1994, p. 258; 1988, p. 124; Hanson 2000, p. 66).

²⁵ “It is true of all armies that, when they are moving into action, the right wing tends to get unduly extended and each side overlaps the enemy’s left with its own right. This is because fear makes every man want to do his best to find protection for his unarmed side in the shield of the man next to him on the right, thinking that the more closely the shields are locked together, the safer he will be.”

²⁶ Krentz (1985, p. 53) has cast doubt upon this traditional understanding, arguing that a hoplite could protect his right side by turning his body. While other of Krentz’s emendations to the traditional view are plausible, he seems to forget that spears were probably only used in the initial stages of battle, and after that, every attacking stroke made by a right hander with a sword would necessarily bring this side of the body forward.

If the strategic importance of such surprise—such standing apart from patterns of regularity and deciding whether to repeat them—comes as no surprise when the topic is a battle, why is it so difficult for us to comprehend when the issue is the place of education in assignment of persons to jobs? Lieberson (1985) argues that a common mistake made in sociological analyses of stratification is to treat a set of regression coefficients as a recipe for success. But, he argues, in perfect agreement with Bourdieu, “Those who write the rules, write rules that enable them to continue to write the rules” (Lieberson 1985, p. 167) and as soon as the dominated (switching to Bourdieu’s language) threaten to assemble the ingredients previously used for success, the recipe miraculously turns out to have changed. If social life is a game, it is not one with unchanging rules.

One clear criticism of field theory is precisely this reliance on seeing social life as an agonistic “game.” While sociological theories may have traditionally downplayed the importance of such agonistic encounters (see Leifer 1988), it is also easy to overstate their importance and to ignore the many social interactions that are certainly nonstrategic and often downright pleasant. Indeed, this seems to be one place where the *Verfremdung* breaks down and certain analysts feel quite comfortable in using their first-person understandings as the basis of theoretical claims. To be blunt, field theorists may go beyond the reasonable assertion that “competition may be legitimately regarded as a basic process in society” (Mannheim 1940, p. 306) and begin generalizing their own anomic social obsessions to all actors in the field. Accordingly, we are right to be suspicious of field theories here.

Clearly there are risks in allowing analysts to use a “game” metaphor to interpret field theory (for a balanced evaluation of game metaphors to describe social action, see Billig [1996, p. 47]; also Nadel [1957, pp. 41–42]). However, the risks—in particular that of overgeneralization—are perhaps not as great as they seem. First, not all human action or behavior takes place “in the field” in that it is judged susceptible to a field effect. Field theorists will tend to assume that action “outside the field” is less analytically interesting, but this remains a prejudice that cannot be directly demonstrated. Many business “friendships,” for example, may be organized according to a logic having to do with the managerial field, and are subject to analysis via field theory, but other friendships may be wholly immune to any field effects and should be understood in other terms.

Second, while it first may seem objectionable that field theorists tend to see social action as a “game” in that it involves striving toward consensually validated goals guided by rules, the use of this term does not necessarily connote triviality. Instead, it maybe a fundamental aspect of social life. Indeed, if people are so attached to rule-guided striving in their

relations to others that they construct unnecessary arenas to act this out and consider the experience pleasurable (“games”), why would we doubt their readiness to organize their actions in such a way when the stakes are higher?

Finally, there is a great strength to the game metaphor, for it suggests a wholly different criterion for successful explanation than the pseudo-causality that is standard in the social sciences. Imagine the position of a commentator on a chess game between two grand masters. Clearly, this commentator must understand the rules that underlie the playing of the game and hopefully is able to remember many other chess games, especially those recently played by the opponents. But to insist that his grasp of the situation must be mirrored in some generalized predictive power—say, the ability to look at the untouched board and declare, “White always wins!”—is utter foolishness. Similarly, a sociological analyst should be able to understand how people act in a field without necessarily having great predictive power, since the better established the rule, the more advantage there may be in breaking it.

Thus the game metaphor for understanding the field as a field of contestation may give us a better understanding of the regularity and irregularity in social life—better, that is, than the conventional partitioning into “explained variance” and “error.” But there is indeed a potential weakness in the game metaphor, in that it simplifies our understanding of the differentiation in persons induced by a field effect. We may consider a “game” to be an intersubjectively valid set of restrictions on interaction that leads to vertical social differentiation among persons. Most simply, this differentiation is into winner(s) and loser(s), although there can be a continuous differentiation as well. However, it is not necessarily clear that fields can only induce such vertical differentiation, and it may be a problem that Bourdieu’s analysis has tended to neglect horizontal differentiation that remains within the field. Horizontal differentiation may be said to exist when persons are not *comparable*, in that it makes no sense to say that one is better than the other according to some field standard nor are they the same, yet for a number of reasons, we need to see them as being in the same field. Thus the game metaphor may introduce an artificial simplification that cuts against the second commonality among field theorists, namely their emphasis on concrete analysis.

Concrete Analysis

I began by noting the confusing readiness of sociological theorists to declare some détente between structure and agency, agreeing to some formula along the lines of “structures constrain but enable.” The field theorists all explicitly shun the basic division of social reality into social law

on the one hand and individual on the other that implicitly underlies all these conceptions. In Lewin's (1999a, p. 65) words, successful analysis "depends upon keeping in mind that general validity of the law and concreteness of the individual case are not antitheses, and that reference to the totality of the concrete whole situation must take the place of reference to the largest possible historical collection of frequent repetitions" (Lewin 1936, pp. 32, 34; also see Brandt 1952, pp. 187, 190; on this emphasis on the concrete see Mohr [in press]).

This statement—like similar admonitions made by Bourdieu—is by no means as trivial as it might first appear. Most sociological analysts tend to assume that examining one concrete case in detail requires a progressive attenuation in importance of "law-like" characteristics and increasing attention to the "unique," "historical," or "accidental" features; this assumption in turn comes from the Durkheimian idea that our laws refer to abstract general qualities common to members of a certain class. Field theory, in contrast, emphasizes that the regularity comes at the level of the situation and that the further one goes into a particular case, the *more* revealing it will be of general principles. The trajectory of a falling feather is not an "exception" to the law of gravity that reveals the "agency" of similar inanimate objects; rather, it is an excellent place to understand the relation between center of gravity and center of pressure. As a consequence, field theory implies a confident pursuit of what Lewin (1999a, p. 58) called "the full concreteness of the particular situation" while conventional approaches fear getting lost in "accidents" (see Cassirer 1953, p. 20). Concreteness, or attention to the particularities of this case, far from being assumed to lead to a "small picture" is inseparable from the field theorists' emphasis on totality and synthesis (cf. Mannheim 1940, pp. 32, 184).

This emphasis on concreteness often seems a refreshing contrast to artificial and abstract *ceteris paribus* situations of the conventional methodology that attempts to "control for" context (see Abbott 1997), but has its own danger. This danger is that a theorist will attempt to formalize more and more relevant factors: something of this is found in Tolman's (1954, p. 287) explanation of going to a restaurant and in many of Lewin's analyses. In hindsight, we see that if a field theory takes everything relevant into effect, and anything may be relevant in certain situations, then no formalization that hopes to be other than (in principle) infinite can introduce theoretical terms pertaining to classes of elements. Hence to the extent that field theory is formalized, it must attempt only to formalize general aspects of the relation between person and field.

To appreciate this, one need only compare the manic and unenlightening figures of Lewin or Tolman (1954)—which include every particular analytic element in question, such as hunger, money, restaurant, menu, stairs,

and so on (so that any figure is different from any other)—to the quite useful figures of Spiegel (1961). Spiegel, in attempting to explain how an “object” requiring public approval (a consumer good, a candidate, etc.) gathers a certain number of adherents and abstainers, only formalizes the position of the object, the distribution of persons in the field, the spread of knowledge about the object, and the overall attractiveness of the object. Here the attempt is not to translate the concrete case to a formal representation *tout court* but to use a reduced representation to make certain general principles (such as that the “gradient” of desirability increases the closer one is to some object) intuitively accessible.

Field theory, one may conclude, is a rather poor vehicle of topographic formalization, and it was an emphasis on such formalization that nearly brought field theory to a premature end in the social sciences with Lewin (see also Mannheim 1940, p. 230). But field theory is an excellent vehicle for making complex social phenomena intuitively accessible without relying on prejudices or “common sense” first person understandings. We have seen field theorists repeatedly stressing such intuitive accessibility (*Anschaulichkeit*) as a key feature of a good theory.²⁷ In particular, field

²⁷ Thus Spiegel (1961, pp. 15–16; cf. Geiger 1949, p. 45) argued that more important than the criteria usually given for model construction is that the model make “a part or an aspect of the reality in question materially or hypothetically intuitively accessible” (*materiell oder ideell veranschaulicht*). He explicitly claimed that his model, though based on a spatialization (*Verräumlichung*) was not an attempt to produce metric or topological statements but merely an intuitively accessible and spatial way of describing (*eine anschaulich-räumliche Darstellungsweise*) using distance metaphorically. Mannheim (1940, pp. 169–71) has perhaps the most complete discussion of the importance of *Anschaulichkeit*, as he sought to compromise between the American abstract-variable approach that focused on isolated causal sequences and the German “intuitive” approach which he associated with Romanticism in general and the German *Gestalt* and developmental (comprehensive) traditions in particular. The former, he argued, excluded the important possibility that we can get valid knowledge of an object through “direct physical and psychic contact and perception.” Mannheim tried to correct the impression that such *anschaulich* science simply uses direct inspection (*Schau*) and intuition (*Intuition*, which Mannheim [1940, p. 232; 1935, p. 202] used instead of *Anschauung* when he wished to denote the “pejorative” or more poetic sense). Far from there being an opposition of the American and Romantic methods of thought, in their most developed form, they both reach toward the same goal—“to grasp the concrete object in its concrete context.” Mannheim’s attempt to bridge the American and German thought styles (interestingly also a goal of Lewin’s and Koffka’s [1935, p. 73]) was a failure (here see Kettler and Meja 1994). This is unfortunately seen in the difference between the English translation (by Shils) and the German original. (Since Mannheim expanded this edition he must be held responsible for its final form, even if the error was originally made by Shils.) Mannheim argued that while the *anschaulich*-intuitive approach does not attempt to *analyze* in the sense of breaking up the object with an eye to its possible reconstruction, this does not mean that the information retrieved cannot be transmitted and formalized, since this knowledge can be brought to (analytic) consciousness. Mannheim (1935, pp. 120–21) is explicit here: “*Das anschauliche Wissen ist keineswegs dazu verurteilt, stumm und unreflexiv zu*

theory elegantly handles as fundamentally the same two social phenomena usually considered to be antithetical, namely the feeling that there is some social force which constrains individuals externally and the feeling that we act on the basis of our motivations.

The conviction that field theorists have that plunging into the concrete will allow anything to be explained leads them to refrain from joining most sociologists as considering true motivations beyond the limits of sociological explanation, something to be invoked only when people are vexingly determined *not* to be explainable. Instead, explanation *necessarily* involves appeal to motivation.

Motivation

Agency, according to the field theoretic accounts, has little or nothing to do with the philosophic “freedom” of the will from sociological determination that has been a tortuous problem for sociological theorists since Quetelet and Comte (see, e.g., Brandt 1952). Instead of sociological generalities constraining individuals externally, field theoretic statements are descriptions of the patterned motivations of persons who are distributed (or have distributed themselves) in some field. Calling them agents (as does Bourdieu) implies nothing as to their philosophical freedom (and this is in contrast to the seemingly similar usage of Giddens).

Thus instead of dreading motivations as things that derail systematic explanations, field theorists assume that the field is defined by certain common primary motivations—subjective representations of “what is good to strive for” such as Bourdieu’s “libido” or Emery and Trist’s (1965, p. 28) “values”—and organizes other ones. This conception differs from standard ideas of values (ultimate or penultimate conceptions of the generally desirable) in two ways, both highlighted by Emery and Trist (who draw upon Lewin’s conception of values). First of all, values are not general aspects of culture, but field specific. Consequently, far from being universal in some group, they are predictably distributed across social positions (Mey 1972, p. 168), since norms are simply a way of describing regularities in motivation that come from interest-locations. Second, while the subjective representations of “what is good to strive for” are generally perceived as ethical imperatives, this ethicality is *secondary* to the field structure—the subjective experience of values as injunctions is a cognitive simplification of what is otherwise a complex task of navigating a field. This principle turns out to be key and requires some expansion.

bleiben” (“intuitive knowledge is in no way accordingly condemned to remain mute and unreflexive”); the 1940 translation [Mannheim 1940, p. 169] says, “intuitive knowledge is for this reason condemned to remain mute and unconstructive.”

A Phenomenology of Intersubjectivity

No sociology can proceed very far without attention to the ways in which people's representations of the world are both similar and different; it also seems quite helpful to be able to understand how people's representations of the world include representations of *others'* representations. On the one hand, traditional approaches stemming from philosophy and social theory lead to tongue twisting as we recognize a distinction between objects in themselves and as they appear to us, requiring us to pay specific attention to the latter (phenomenology). On the other hand, a moment of nonarbitrariness is introduced by the requirement of intersubjective communication: our representations must have some degree of overlap, although unless one embraces Kantianism this agreement is difficult to explain. Contemporary approaches to the problem attempt a consistent answer either by emphasizing intersubjectivity of conceptual patterns while downplaying the connection of these patterns to noumena (now loosely called "constructivism") or by downplaying the importance of intersubjectivity by emphasizing that there is no real distinction between noumena and phenomena (often called "positivism" for no clear reason).

While Bourdieu has emphasized the futility of this opposition, he has at times suggested that a "both/and" solution suffices—that is, that "constructivism" and "objectivism" need only be wedded in a back-and-forth analysis. But the principles of field theory, and hence almost all field theorists, reject the assumption that there is in principle a divorce of phenomenology from objective description (cf. Mohr, in press). Indeed, it was a conviction that the phenomenologically perceived world should be treated as valid that led to the emergence of *Gestalt* psychology and the first field investigations. In Köhler's (1947, p. 21) words, the world we inhabit is "so absolutely objective . . . that for a more objective world no place [is] left."

The rejection of the behaviorists' distinction between the objective description of the scientist and the naïve understandings and reports of the subjects was motivated not by philosophical niceties but by the need to account for people's ability to navigate the field. Such explanation forces us to admit that while the field is a phenomenological construct that must be described as such, it partakes of all the regularity and structure that could be desired (cf. Mead 1938, pp. 610–11, 612). As Spiegel (1961, p. 30) says, "Reality in the social field is just the phenomenal, the immediately encountered." This is because the field is necessarily an intersubjectively organized phenomenological construct. Our mapping of the field is not arbitrary, but neither is it complete: instead, it is a retrieval of information for action, organized with respect to one's current position.

"A cliff looks dangerous—and it is!" writes Gibson (1986). It looks—

and feels—more and more dangerous the closer one is to it. In other words, we directly retrieve from the environment an imperative for action: get away. There is no need to categorize and reflect; the visceral impulse to move away is anterior to the cognitive understanding. Consciousness, to take a term from Whitehead ([1929] 1978) and Mead (1926, p. 76), is a “prehensive” faculty in that the concepts it creates are grasped from the nature of the surrounding world; we are best off considering these action imperatives qualities of the (phenomenological) objects themselves (Koffka 1935, p. 379). Similarly, our understanding of the social field is both direct and couched in the socially constructed categories that are relevant for our conduct. Consequently, field theory implies an understanding of social perception that flies in the face of the dominant pseudo-Durkheimian orthodoxy (exemplified by Mary Douglas [1986]) that sense data come in relatively disorganized form and are sorted according to a culture that, in James’s ([1909] 1975, p. 7) words, throws “‘categories’ over them like a net.”

In contrast, field theory implies a very different account (though this is not always fully worked out). Instead of being fundamentally disorganized, percepts are fundamentally organized because they come from a world or environment with its own principles of organization.²⁸ The task of the perceiver is to establish what Bourdieu calls an “ontological complicity”—to take advantage of the preexisting structural principles of the social order, what Emery and Trist (1965, p. 22), following Tolman and Brunswik, called the “causal texture of the environment.” Thus instead of having a complete (and prohibitively complex) cognitive map that allows one to take any unique object and fit it into a set of categories (if possible), people are assumed to have a simple ability to tell what is “demanded” from them at any time. “As a rule, things are what they look like, or otherwise expressed, their looks tell us what to do with them”—“a fruit says, ‘Eat me’” (Koffka 1935, pp. 76, 7, 353, 356, 392–93; cf. Ushenko 1958, p. 90). The organization to actors’ concepts thus comes from the environment, and not from themselves (cf. Simon 1996). The cognitive tasks they carry out are not cultural schemes that independently exist within their psyches, but merely minor completions to what the environment already “affords” (also see Swidler 1992; Hutchins 1995; White 1992; Gibson 1986, p. 246). In this rejection of the idea that either everyday cognition is inherently problematic or it is simple positivist knowledge, and its emphasis that knowledge is always “for something,”

²⁸ As Koffka (1935, p. 67) argued, “The environment is neither a mosaic of sensations nor a ‘blooming, buzzing confusion,’ nor a blurred and vague total unit; rather does it consist of a definite number of separate objects and events, which, as separate objects and events, are products of organization.”

field theory comes close to the American pragmatic tradition (e.g., Mead 1938, pp. 35, 43) that has also been wedded with “Cassirer-ism” to produce relational analysis by Emirbayer (1997; see Mannheim 1940, p. 206 for an appreciation of this aspect of pragmatism).

Further, field approaches tend to wed perception and apperception (perception of one’s own person as perceiver) in a theoretically rich dualism (as opposed to the nontrivial, but wholly formal, connection of Kant [(1787) 1950]. Bourdieu (1985*b*, p. 728) calls this “a sense of one’s place” while Gibson (1986, p. 183) calls this interspecific visual information as opposed to exterospecific, but in both cases the idea is that the process whereby the person takes in information about the world is not only *relative* to this person’s position in the field (as in the early work of Mannheim [1936]), but simultaneously gives this person information about his or her own position (as in Mannheim 1940, p. 212–13; cf. Köhler 1947, p. 297). This understanding of cognition as a seamless web of extero- and interspecific information about an intersubjectively valid field allows field theory not only to explain the correspondence of social thought to social location (previous answers in terms of interests being psychologically implausible) but to open the way for a more general understanding of the relation between subjective understandings and trajectory through a social environment.

The commonalties in field theory suggest that some of the recent advances made in terms of the field theory of organizations may have general application: I close by suggesting one such direction of generalization.

DISCUSSION

Fields and Institutions

Institutions are frequently considered to be the building blocks of social explanation, yet there has been a fundamental ambiguity as to what institutions are or, more precisely, what should be their role in sociological explanation. Since the most common definition of an institution is a pattern of regularized conduct, we might conclude that institutions are best studied in terms of observable regularities.

Yet analysts tend to assume that the most important thing about institutions is not that the observer can see the regularity, but that the participants can. In the formulation of Parsons and Shils (1954), each interactant has expectations of the other. From this perspective, the thing that is distinctive about an institution is not that it is regular (there are patterns of conduct whose regularity, although visible to an acute ethological observer, is missed by the participants themselves). Instead, what

is distinctive is that you know, when interacting with some type of person or in some setting, more or less what to do.

We may take as a perfect example of such an institution “marriage”—this institution is not defined in terms of concrete patterns of conduct between particular persons; rather, we say that it is an institution because if particular people do get married the range of arbitrary actions they could take is considerably narrowed (Swidler 2001; Turner 1974, p. 17). While they need not fulfill any particular expectations, nonfulfillment will have a very clear semiotic import, whether or not this is intended. Normative accounts have had difficulty in explaining how persons manage to walk around with the vast catalogues of rules they are supposedly following. Yet scoffing at the implausibility of such accounts—and pointing to the fact that the nonfulfillment of norms can be explained neither by randomness nor by orneriness—does not answer the remaining question of why social life *is* as regular as it is.

Field theory suggests that such regularity may be more parsimoniously explained as an internalization of field position. That is, if institutions are about the knowing more than the doing, but it seems implausible that persons carry around large sets of instructions that they do not seem to wholeheartedly believe, it may be that the overall coherence arises because the institutions are linked in terms of some larger field. This linkage then allows for a coherence of subjectivity when moving from one institutionalized form of action to another. In the cases analyzed by Bourdieu, this coherence arises because all the institutions are related to striving of some form. But this is perhaps not necessary. If “marriage” is an institution and “parenthood” also one, it is reasonable to think that these two institutions are easily aligned in that there is relatively little dislocation of subjectivity (which is not to say absence of conflict) when progressing from one to the other. Indeed, the connection between certain institutions (e.g., high school on the one hand, and both vocational school and the university on the other) may owe more to such consistency in subjectivity than to actual organizational interface.

The phrase “married with children” makes a different sort of sense from the phrase “married with employees”—the former is more easily reduced to a single cognitive chunk in contemporary society. Of course, where the household (in the ancient sense) was the center of production, this would not be the case. If indeed there is something parsimonious in the phrase “married with children,” it is, I propose, because this refers to a meaningful position in a field that references a set of consistent affordances, demands for action that may be contradictory but coexist in a coherent phenomenological world.

In sum, most generally, we may say that a field exists when a set of analytic elements are aligned in such a way that it is parsimonious to

describe their current state in terms of position vis-à-vis one another. While this alignment has been most clearly worked out for the case of organizations, it may be that the basic insights can be extended to the case of institutions more generally. That is, organizational fields connect and align organizations, and in so doing, can induce shared subjectivities, or “culture” as we call it (see Meyer 1987; Meyer and Rowan 1977; Meyer, Boli, Thomas, and Ramirez 1997). In analogous fashion, it may be that interinstitutional fields produce such regularities in orientations.

We have seen that interorganizational fields arise when, for example, a set of organizations shares suppliers and clients. More generally, fields may arise, as Fligstein (2001) recently proposes, whenever groups of actors frame their action vis-à-vis one another. For example, fields in this slightly broader sense arise whenever a set of individuals are striving after a similar goal. Most generally, we may say fields emerge whenever we find a set of institutions that individuals tend to traverse in predictable ways with minimal dislocation of subjectivity. In all cases, the field is something that spans and coordinates institutions by allowing individuals to understand their past, current, and future situations in terms of position, trajectory, and similarity or closeness (see Turner 1974, p. 139; cf. Mohr 1994).

As a result, one need not always examine the myriad interpersonal relations to understand the context of action as suggested by Emirbayer’s recent (1997) manifesto. For when a field exists (and it is worth emphasizing that fields do not always exist), these relations are simplified to the point where participants can successfully plan their action by summarizing these relationships into a single position (making an otherwise cognitively daunting task possible for those of us with bounded rationality). Further, field theory makes the exciting, nontrivial, and generative claim that action can be explained by close attention to field position as every position in the field induces a set of motivations that are subjectively experienced as “what should be done.” Field theory disappoints us in remaining vague as to precisely how this occurs and we hope that it can be eventually surpassed in this regard. Yet it promises the chance of combining rigorous analytic insight with attention to the concrete.

Conclusion

First of all, an abstraction is made from a fact; then [the fact] is based upon the abstraction. That is how to proceed if you want to appear German, profound and speculative.—Karl Marx and Frederick Engels, *The German Ideology*

I have admitted that field theory is in places necessarily indeterminate

and resists formalization. Given that we already have a theoretical tradition implicit in our methodology (though one that is not codified and propounded by many contemporary self-identified theorists) that is compatible (in principle) with perfect determination and formalization, it is reasonable to ask whether the trade proposed is a good one. In response, we must first ask, does our current overarching theoretical approach do what one would demand of any scientific endeavor, namely that it explain something? Of course, “explanation” is what conventional sociology is all about. But this sense of “explain,” far from being a universal scientific one, is peculiar to the contemporary social sciences and laden with paradox. It derives more from the substantive implications of our methodological tools than from any understanding of scientific method, and these implications are generally ones which almost no sociologist would be willing to defend.

One “explains” something in this sense by linking variation in one attribute of some units to its variation in their other attributes. Invoking the catch phrase that “correlation does not imply causation,” we then consider our explanation a true one if our association can be linked to a pseudocausal model in which—to take the claims seriously—one attribute of persons (say) *causes* another attribute of those persons (who presumably have little to do during this whole process) (see Abbott 1992).

In this understanding, explanation is a relationship between a researcher and a phenomenon—in many senses, the term “explanation” could be replaced with “disappearance,” as the analysts’ goal is to make some unaccounted-for phenomenon go away as a thing in itself (cf. Koffka 1935, p. 178f). Once the R^2 approaches 1, “explanation” has occurred because there simply *is* no phenomenon *sui generis* anymore. Of course, few researchers attempt to obliterate the phenomenon in this fashion, and many use the underlying paradigm in unorthodox ways. Yet the basic understanding of what it means to explain is formally parallel to the thinking denounced by Marx and Engels: first variables are made out of people, and then the actions of the people are “explained” on the basis of the variables (cf. James [1907] 1946, p. 263). I propose that this is actually no explanation at all.

In contrast, field theory takes explanation in a sense so obvious and unambiguous that it has become difficult for social scientists to take seriously: explanation is a social relationship between people, in which some phenomenon is explained to some person(s) so that they understand it (cf. Lundberg 1939, p. 51). The criteria of successful explanation given this definition are, of course, far more fuzzy than those that come with the notion of explanation as some act of ontological aggression against a phenomenon of interest. But this in itself does not justify rejecting this definition of explanation. Indeed, it is hard to see why anyone could call

research “explanation” that does not “make plain,” or, equivalently to the field theorist, situate in an analytic plane with interpretable characteristics.

Above I compared field theory to commentary on a chess game; this comparison is apposite not so much because of what it implies about the nature of social action, but because of what it implies about the nature of sociological explanation. To explain social action is akin to explaining why a grand master allowed a knight to exchange for a pawn, as opposed to considering this an error on the order of $3 - 1 = 2$ (the difference in points between the two). We cannot say in advance how far we must go in our quest to determine the context that led to this move, but this indeterminacy is simply the minimal flexibility required to understand complexity.

Other researchers, less wedded to the conventional notion of explanation, may, like Molière’s Monsieur Jourdain, feel that they have been doing field theory all their lives.²⁹ One would hope that this is the case: if sociologists were not currently able to carry out substantive investigations along the lines proposed here, it would be rash to suggest a coherent theoretic approach. But even contemporary researches that already follow more or less along the lines of field theory may benefit by having a coherent defense of their explanatory principles, as opposed to having to rely on ad hoc and apologetic justifications of their deviation from causal analysis via variables.

Finally, field theory may have suggestive implications for the question of how we integrate history with synchronic explanation, since Lewin’s “principle of contemporaneity” actually leads us to emphasize the heritage of the field. At any time, institutions exist in some form of alignment which lets us orient our action in an intersubjectively comprehensible way; as Bourdieu has repeatedly stressed, that our action may attempt to dismantle or skirt this current form of alignment is not at all contradictory and introduces no conceptual difficulties. If this is so, it might be the case that precisely because field theory rejects the direct influence of the past, we find that social action is saturated with history (see Bourdieu 1993, p. 74). If action is understood as a field effect, then we cannot decompose it into rational or functional on the one hand and vestigial on the other (as did Spencer most famously). Not only is it mistaken to assume that people choose their allies and tactics, in many cases there is no reason to assume that they choose their goals. This is not because, as critics of Bourdieu have sometimes assumed, field theory implies that people are cognitively limited in their vision of what they want. It is because the only way to reach conditions that we cognize and wish for is to make use of those conditions that we have not wished for. It may be social actors

²⁹ I thank a reviewer for pointing this out.

are largely in the position of the soldiers of Fredonia in the Marx Brothers's *Duck Soup*, who resolve their conflict with their neighbor but decide to go to war anyway since they have "already put down two months' rent on the battlefield."

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